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Controversial Findings are Important to Managers

J. Scott Armstrong
University of Pennsylvania, armstrong@wharton.upenn.edu

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Abstract
Comment on the purpose of publishing management research.

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replications." Richard Franke convinced me that the problem had broader implications. One of the primary functions of management scientists is to objectively examine management practice and to report on the value of these practices.

I am indebted to Peterson because he accepted our paper on escalation despite negative reviews. He has a great deal of experience as an editor, knows the research on publication, and has improved the reviewing process when he has served as an editor. I agree with his statement that "the most important decision in the peer review process is selecting the editor." (Some evidence exists on this issue; Franke, Edlund, and Oster's [1990] analysis of 17 management journals over a 12-year period led them to conclude that the journal quality was higher if the editor was a successful researcher.) The editor can change reviewing procedures, select referees, and make the decision about whether to publish a paper. I also agree that much can be done to "train and motivate reviewers." Two useful approaches in this regard are the use of formal rating sheets and prepaid honorariums for completing the review by a given date.

While we agree on many things, we differ about the purpose of publishing management research. Peterson says that "Providing equity in the peer review process is the primary responsibility of an editor." In contrast, I believe that the primary objective of an editor should be to ensure the publication of important new findings that might improve the practice of management. That is, I would put more emphasis on usefulness to science than on fairness to scientists.

Peterson's viewpoint probably has more

Controversial Findings Are Important to Managers, a reply by J. Scott Armstrong.

Robert Peterson believes that the title of my paper is misleading. He views that paper as a criticism of the peer review process. It is, of course. In fact, my original title contained the term "controversial

supporters than mine. According to Horrobin [1990], the most common assumption among those who study the peer review process is that the purpose is "quality control." Horrobin believes that this assumption is counterproductive and that the primary focus should be on the possible benefit of the research findings. He reviews critical cases where peer review suppressed important findings.

Our differences about the basic emphasis (fair as opposed to useful) lead us to differ about blind reviewing (blind in that the reviewer does not see the author's name). Blind reviewing is useful for helping new researchers or researchers from low prestige institutions. However, authors should be free to reveal their identities by citing their previous work. One of the best predictors of whether a person can successfully conduct research is that person's prior success as a researcher. Abrams [1991] found that authors who were highly cited on their earlier work were also highly cited on their later work. This suggests that a researcher's prior record should be a good predictor of success on a new project. Peterson raises the possibility that these researchers would merely support their existing paradigms. Thus, I should qualify my suggestion to state that researchers who have a history of successful discoveries should receive special consideration when they have findings that challenge folklore. In a sense, this would allow well-respected researchers to risk their reputations.

The primary objective of an editor should be to ensure publication of important new findings.

I appreciate the arguments for "fairness" in blind reviewing, but I doubt that bias against authors (or their affiliations) causes any significant harm to scientific advancements, although I do not know of any evidence on this issue. On the other hand, I suspect that bias against new findings is a major barrier. That said, I am not against procedures used to promote fairness, and Peterson is not against procedures that promote the publication of new findings.

Peterson suggests that most readers can probably relate to the criticisms in my paper. Interestingly, one researcher who read an early draft of the paper said just the opposite. Most researchers, he said, do not obtain findings that are controversial in any way, so they will probably wonder what these papers are all about.

Peterson states that these issues have been raised before. While this is true, I am impressed by how much has been learned in the past two decades through empirical research on the scientific process. The vast majority of the studies that I cited were published in the past two decades. In addition, there is the issue of how to apply this knowledge. One purpose of my paper was to use these findings to examine ways to challenge folklore.

Peterson says that editors are author-friendly. I agree. In fact, were it otherwise, I would not have had much of a career. Except for a few of my less important papers, my submissions typically cause one or more referees (and many readers) to become irate. When I consider what I believe to be my best work, I cannot remember a single paper that was favorably reviewed

by all referees. Those who challenge folklore owe much to editors like Peterson.

With respect to Franke's comments, the historical viewpoint leads one to ask whether things are improving. Although I cannot provide empirical support, I believe that the increase in the number of journals has helped to break down barriers to controversial findings. In addition, electronic publishing should reduce the time and costs for the dissemination of scientific findings. Of course, researchers will need some way to gain credit for their findings, and it is not clear how this will be done with electronic publishing.

I believe that the only good argument for denying researchers the right to present their findings is that it costs too much. There are only so many journal pages and decisions must be made about what to reject. This argument does not hold for conferences. Conferences typically gain net revenues as they add presenters. Thus, conferences do not need peer review, unless the organization must decide which papers should have more time and better time slots.

Arkes suggests that editorial boards, rather than editors, make the decisions. I propose an extension to this. That is, rather than seeking consensus among the board members, a paper would be published if one board member were willing to act as its advocate. The advocate(s) would be identified when the paper is published, along with a note from the editors. Each board member would be permitted to advocate a limited number of papers each year. This procedure might publish more papers that are incorrect, but it should increase the likelihood of publishing papers that successfully challenge folklore. These should help managers to improve their effectiveness.

References

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